and letters from him; and, as he has acknowledged in his book, I carefully studied these specimens and gave him all the advice and assistance in my power in carrying on his geological investigations. Upon his return we had several conversations, always of the most friendly character, concerning the best mode of embodying his observations for communication to different Societies; and until the present time I had not the smallest idea that he was in any way dissatisfied with anything I had ever said or done in connection with the subject.

With respect to a particular conversation referred to by Dr. Guppy as having occurred in the spring of 1885, I have no recollection whatever; but I unreservedly accept his statement as to the *facts* of the case, and only demur to his *interpretation* of them. If Dr. Guppy or anyone else asked my opinion as to the fitness for the Geological Society of "a paper in which Mr. Darwin's theory of coral reefs would be brought under consideration," I should undoubtedly point out to him that the Geological Society had always been averse to publishing papers dealing with such broadly theoretical questions as the origin of coral reefs, and I should advise some other means of publication as more appropriate.

That the Geological Society is not partial in its reluctance to publish papers of a theoretical character will be seen from the fact that although Mr. Darwin in 1837 read a paper to the Society, embodying the chief points of his theory of coral reefs, yet the Society never published the paper in their Transactions. At the time that this occurred Darwin was a member of the Council, and a few months later he became Secretary of the Society.

It is surely unnecessary for me to remark that in any advice which I gave to Dr. Guppy, I was acting simply on my own judgment and individual responsibility. Dr. Guppy was a Fellow of the Society at the time, and had precisely the same right to present papers to it which I had myself. Dr. Guppy chose to ask my advice; I gave it to him as to a friend, and he was perfectly free to act upon it or to reject it as he thought fit. I may add that the Secretary of the Geological Society has nothing to do with the acceptance or rejection of papers, except as a member of the Council, and then only when the question has been brought up by the President for the approval or condemnation of his own action.

How my unofficial act of friendly advice concerning the destination of an unwritten paper came to be represented as the refusal of a paper offered to the Society I am at a loss to conceive! Why the Duke of Argyll, having received the statement which is now before us, should proceed to formulate the very grave accusation against Prof. Bonney personally, and the authorities of the Geological Society, it is for his Grace to explain.

With respect to Dr. Guppy's complaint that his memoirs have been "studiously ignored" during the recent controversy, I cannot help thinking that he has been unduly sensitive. Writing to Prof. Huxley in October last year, and pointing out that the Duke of Argyll was mistaken in supposing there had been no discussion on Mr. Murray's theory, I said "it would be an endless task to attempt to give references to the various scientific journals which have discussed the subject," but in penning these words I had not the smallest idea of speaking slightingly of any of the memoirs which want of space prevented me from citing, and least of all concerning those which contain the facts and observations of Dr. Guppy, of the value and importance of which I had such good opportunities of judging.

JOHN W. JUDD.

Reason and Language.

PROF. MAX MÜLLER has been so kind as to favour the readers of NATURE with his views on language and reason, concisely expressed in a letter to an American friend. As one grateful reader, I much desire both to express my thanks, and also to beg for yet a little further information with respect to matters of such extreme interest.

The Professor says: "Because we reason—that is, because we reckon, because we add and subtract—therefore we say that we have reason." Now, in the first place, I should be glad to be told why "reason" is to be regarded as identical with such "reckoning"? I have been taught to distinguish two forms of intellectual activity: (1) acts of intuition, by which we directly apprehend certain truths, such as *e g.* our own activity, or that A is A; and (2) Acts of inference, by which we indirectly apprehend others, with the aid of the idea "therefore"—evolving into explicit recognition a truth previously implicit and latent in premisses. The processes of addition and subtraction alone, seem to me to constitute a very incomplete representation of our mental processes.

The Professor also identifies language and reason, denying to either a separate existence. As to "reason" he says: "We have only to look into the workshop of language in order to see that there is nothing substantial corresponding to this substantive, and that neither the heart nor the brain, neither the breath nor the spirit, of man discloses its original whereabouts." The expression "whereabouts" would seem to attribute to those who assert the existence of "reason," the idea that it possesses the attribute of extension! In order to understand clearly the passage quoted, we should learn what Prof. Max Müller really means by the term "spirit," which here figures as one species of a genus also comprising the breath, the brain, and the heart. Reason, however, is not represented as being simply language "as we now hear it and use it," but "as it has been slowly elaborated by man through all the ages of his existence upon earth." Thus understood, the Professor "cannot doubt" "the identity of reason and language." Nevertheless, he immediately proceeds to point out a striking want of identity between them. He says, quite truly, "We have two words, and therefore it requires with us a strong effort to perceive that behind these two words there is but one essence,"—namely, that denoted by the Greek word *logos*—"the undivided essence of language and thought." Now, the intimate connection of language (whether of speech or gesture) with thought is unquestionable; but intimate connection is not "identity." If thought and language are "*identical*," how came two words not to have two meanings, or two thoughts to be expressed by one word? The plain fact that we have different words with one meaning, and different meanings with one word, seems to demonstrate that thought and language cannot be "identical."

"No reason without language—no language without reason" is a statement true in a certain sense, but a statement which cannot be affirmed absolutely. Language (meaning by that term only *intellectual* expression by voice or gesture) cannot manifestly exist without reason; but no person who thinks it even *possible* that an intelligence may exist of which ours is but a feeble copy, can venture dogmatically to affirm that there *is* no reason without language, unless he means by reason, mere "reasoning," which is evidently the makeshift of an inferior order of intellect unable to attain certain truths save by the roundabout process of inference.

But I demur to the assertion that truly intellectual processes cannot take place in us apart from language. In such matters our ultimate appeal must be to our own reflective consciousness. Mine plainly tells me that I have every now and then apprehensions which flash into my mind far too rapidly to clothe themselves even in mental words, which latter require to be sought in order to express such apprehensions. I also find myself sometimes expressing a voluminous perception by a sudden gesture far too rapid even for thought-words, and I believe that other persons do the same. A slight movement of a finger, or the incipient closure of an eyelid, may give expression to a meaning which could only be thought in words by a much slower process.

It is the more remarkable that Prof. Max Müller should deny the existence of reason, since he unequivocally affirms, in rather lofty language, the existence of truth. Yet surely the existence of truth, in and by itself, is inconceivable. What can truth be, save a conformity of thoughts and things? I affirm, indeed, the certain existence of truth, but I also affirm that of reason, as existing anteriorly to language—whether of voice or gesture. What is the teaching of experience? Do men invent new concepts to suit previously coined words, or new words to give expression to freshly thought-out concepts? The often-referred-to jabber of Hottentots is not in point. No sounds or gestures which do not express concepts would be admitted by either Prof. Max Müller or myself to be "language."

The Professor speaks of the "alarmingly small" number of primitive concepts; but who is to be thereby alarmed? Not men who occupy a similar stand-point to mine. I fully agree with Prof. Max Müller in saying, "After the genesis of the first concept, everything else becomes intelligible."

We come now to the supreme question of the origin of languages. As to this, the Professor observes: "No one who has not himself grappled with that problem can appreciate the complete change

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that has come over it by the recognition of the fact that roots are the phonetic expressions of the consciousness of our own acts. Nothing but this, our consciousness of our own repeated acts, could possibly have given us our first concepts. Nothing else answers the necessary requirements of a concept, that it should be the consciousness of something manifold, yet necessarily realized as one. . . The results of our acts become the first objects of our conceptual thought." The truth of these statements I venture to question. After noting the dogmatic nature of the assertion "Nothing but this *could*, &c.," I must object to the statement of fact as regards human beings now. I do not believe that the infant's first object of thought is "the results of its own acts." In the first place, no object of our early thoughts is merely the "results of our own acts," but a combined result of our own activity and of the action on us of our environment. Secondly, my observations lead me to believe that the infant's first thoughts relate to things external, and certainly not to the results of its own activity as such, which is a highly complex and developed thought. It may be that the Professor, when he says "The results of our acts *become* the first object of our conceptual thoughts," means that such acts in remote antiquity *became* the object of man's first thought. This is probably the case, since, with respect to the origin of thought and language, Prof. Max Müller has adopted Noiré's crude notion that they sprang from sounds emitted by men at work, conscious of what they were doing, in the presence of others who beheld their actions and heard the sounds; the result being the formation of a conceptual word, to attain which five stages had to be gone through, as follows:—

"(1) Consciousness of our own repeated acts.
"(2) *Clamor concomitans* of these acts.
"(3) Consciousness of our *clamor* as concomitant to the act.
"(4) Repetition of that *clamor* to recall the act.
"(5) *Clamor* (root) defined by prefixes, suffixes, &c., to recall the act as localized in its results, its instruments, its agents, &c."

But, if language and reason are identical, reason could not exist before a single conceptual word existed. Nevertheless, to attain to this first single word, we see, from the above quotation, that man must have had the notion of his own acts as such; the notion of their repetition; the notions of clamor, action, and the simultaneity of clamor and action; the will to recall the act (yet *nihil volitum quia præcognitum*); and finally the notions of consequence, instrumentality, agency, or whatever further notions the Professor may intend by his "&c."

Thus he who first developed language must be admitted to have already had a mind well stored with intellectual notions! But can it for one instant be seriously maintained, close as is the connection of language with reason, that their genesis (miracle apart, of which there is no question) was *absolutely* simultaneous? He must be a bold, not to say a rash, man who would dogmatically affirm this. But if they were not *absolutely* simultaneous, one must have existed, for however brief a space, before the other. That intellectual language could have existed without reason is absurd. Reason, then, must, for however short a period, have preceded language.

In conclusion, I desire to point out a certain misrepresentation with respect to natural selection. The Professor says: "In the evolution of the mind, as well as in that of Nature, natural selection is rational selection; or, in reality, the triumph of reason, the triumph of what is reasonable and right; or, as people now say, of what is fittest." But, we may ask in passing, if reason has no existence, how can it "triumph"? The misrepresentation of natural selection, however, lies in his use of the word "fittest." When biologists say that the "fittest" survives, they do not mean to say that that survives which is the most "reasonable and right," but that that survives which *is able to survive*. What there is less "reasonable and right" in a Rhytina than in a Dugong, or in a Dinornis than an Apteryx, would, I think, puzzle most of our zoologists to determine; nor is it easy to see a triumph of reason, in the extermination of the unique flora of St. Helena by the introduction of goats and rabbits. ST. GEORGE MIVART.

Mechanical Equivalent of Heat.

I FIND that the mode of regarding J advocated in my letter in last week's NATURE (p. 320) is not quite new, for my brother, Dr. Oliver Lodge, writes to tell me that Clerk-Maxwell, on p. 298 of his "Theory of Heat," has called J the specific heat of water. However, he has not done so throughout the book, and I do not think it is the meaning generally attached to the symbol, though it seems to me that it should be so; that is to say, J should always be considered as denoting the specific heat of water at the temperature 0° C. ALFRED LODGE.

Coopers Hill, Staines, February 6.

"Is Hail so formed?"

I CANNOT accept Dr. Rae's explanation as a "simpler solution" of the phenomenon described by me in NATURE of January 26 (p. 295), because it is based upon meteorological conditions that were at the time non-existent.

My own observation of the pine-tree convinced me that at or near the summit there was no adherent ice or rime; and had there been beads of ice upon the leaves I should still have failed to see what should have caused them while frozen to become detached and change from beads to pellets.

There was a fine mist during the whole of the day, and I observed the phenomenon at 3.30 p.m.

A letter appeared in NATURE upon the same day as mine, drawing attention to the unusual atmospheric conditions observed about that time, and containing facts which manifestly support my theory. CECIL CARUS-WILSON.

Bournemouth, February 11.

The New Army Regulations.

THE new regulations for the Woolwich entrance examination have been very unfavourably received by men of science. This hostile criticism is in some respects the consequence of the absence of clear discrimination between them and those already in force for the Sandhurst examination.

It must be remembered that candidates for Woolwich cadetships must be between the ages of 16 and 18; that 6000 marks are awarded for mathematics, with 1500 more for drawing and English composition; and that in both the last June and December competitions less than 4000 marks sufficed to place a student among the successful competitors. Since candidates can pass in these subjects alone, it appears unreasonable to complain that youths of scientific power are excluded from the Royal Military Academy. Classics are sufficiently discouraged by the fact that they have no mark value after the cadet has entered the Academy. The 5000 marks offered in the entrance examination for Latin and Greek merely serve to encourage candidates who have been educated on the classical sides, which are almost always the stronger at our public schools. They really tend to widen rather than to narrow the sources from which candidates are drawn.

After a quarter of a century of continuous experience as a student and teacher of elementary science, I find myself reluctantly forced to the conclusion that chemistry, physics, and geology are not good educational subjects for lads under 16 years of age. I believe that it is in most cases desirable that youths intended for a scientific career should not specialize too early. A sound foundation of mathematics and modern languages is almost necessary to enable them to attack their scientific subjects efficiently. With minds trained to the use of the exact and powerful processes of mathematical reasoning, and able to readily appreciate and avail themselves of the wealth of scientific literature in France and Germany, they will probably become more useful officers than if they had acquired a smattering of science.

On the other hand, your wise censure of the discouragment of science in the Sandhurst regulations must commend itself to all thoughtful men. The case is even stronger than at first sight appears in the studious moderation of your judicious article. The limits of age are higher for Sandhurst, being 20, or in some cases 24. The training of the Line cadets is less complete. As they only spend one year at Sandhurst, they are obliged to confine their attention more strictly to professional subjects. Officers of the Line have often more leisure than those in the scientific corps, and there are many reasons why even a slight acquaintance with science would be helpful to them. It also seems hard that a candidate should be handicapped by not taking up Latin. Sometimes it has been discontinued for a considerable period, and a candidate can ill afford to take up "a 2000 subject," considering the severity of the competition.

I would wish respectfully to suggest that a memorial should be presented to the War Office by all interested in the teaching of science, praying that, if a candidate for an army examination

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